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AUTHOR Mishler, Elliot G.
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ABSTRACT

The scientific method is described as intending to be the guarantor of objectivity, neutrality, and diligent application of its rigorous procedures and to produce a body of cumulative knowledge that is systematic and general. The central argument of this essay addresses the discovery that research findings appear to be context-dependent; methods of the traditional model of science are inappropriate to the study of context-dependent phenomena. Models that may provide a better fit between research methods and phenomena of interest, namely the context-dependence of human meaning and action, are described as available and deserving of attention. A number of these alternative approaches and methods that may be more appropriate are described. These methods are drawn from ecological psychology, phenomenological research, sociolinguistics, and ethnomethodology. It is concluded that these methods vary among themselves, and each has its own problems, but they do provide a range of possibilities for rethinking ways of conducting scientific research. (Author/RD)

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Meaning in Context: Is There Any Other Kind*

Elliot G. Mishler

Department of Psychiatry, Harvard Medical School

and the Massachusetts Mental Health Center

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My title is "Meaning in Context: Is There Any Other Kind?" The tag question was intended, of course, as rhetorical and I am sure that it was understood in this sense by everyone. Further, to anticipate the theme of this paper, I'm sure that your understanding of it as rhetorical was grounded in your knowledge of the multiple contexts within which the question appears. The symposium itself, with its playful reversal of a folk-saying, is one such context and we participants are clearly intended to put the horse in its proper place, namely, before the cart. There is also the larger context of this meeting, a meeting devoted to educational research, and related research in the social and psychological sciences. The double irony contained in the reversal of the folk-saying and in the rhetorical question, both unusual meta-comments in titles of scientific papers, suggests that a critical perspective is to be applied to some aspect of this larger context. You will already be expecting, from the title alone, a critique of a particular genre of research perhaps, or of classroom interaction studies or evaluation research, or perhaps of the misuse of certain statistical procedures. These and other much more complex understandings and anticipations have all been generated with extraordinary speed by your being able to understand the manifold of contextually-grounded meanings carried by that simple five-word tag question --Is there any other kind?

If so much meaning can be produced by so little, by a five-word utterance marked as a question, think of how much more complex and elaborated our understanding is likely to be of utterances or actions embedded in more complex, more ambiguous, and more swiftly moving streams of social interaction. The irony in the question, an irony carried by the fact that

you hear it as rhetorical and I intend it as such, points to a paradox that will serve as a point of departure for this essay.

One side of the paradox is that we all know that human action and experience are context-dependent and may only be understood within their contexts; that is why the question is rhetorical. We understand what others are doing and what they mean by what they say by seeing and hearing them in context; we rely equally and necessarily on context for our own behavior to be understood and meaningful to others, and we implicitly ground our interpretations of each others' motives and intentions in context. This is neither a surprising or original observation; it is integral to our everyday consciousness and awareness, an essential feature of our "natural attitude." (cf. Garfinkel, 1967)

The other side of the paradox is that this ordinary and common-sense understanding of meaning as context-dependent has been excluded from the main tradition of theory and research in the social and psychological sciences. This is the reason that the question is heard as ironic as well as rhetorical. In our special stance as theorists and researchers, we have tended to behave as if context is the enemy of understanding rather than the resource for understanding which it is in our everyday lives.

This point is clear in both our professed theoretical goals and in the research methods designed to implement them. Our ideal in theoretical work is the development and formulation of general laws, hopefully laws that are universal. The essential feature of such general and universal laws is that they be context-independent, that is, free of the specific constraints of any particular context and therefore applicable to all. If I am correct in assuming that we all share an understanding of meaning

as context-dependent, then it will be evident why the search for such laws has proved so difficult.

This conceptual difficulty has not deterred methodologists. In pursuit of general laws, their efforts have been directed to the stripping away of contexts. Context-stripping is a key feature of our standard methods of experimental design, measurement, and statistical analysis. For example, in order to test the generality of our hypotheses, we remove the subjects of our studies from their natural social settings; their normal roles and social networks are left behind as they enter our experimental laboratories much as we leave our shoes outside on entering a shrine. They are then randomly assigned to different experimental treatments, as if they were as interchangeable as different seeds of corn or alfalfa. Thus, R.A. Fisher's elegant methods of agricultural experimentation are carried over into the human sciences, bringing with them the context-free assumptions of the agricultural sciences.

Further, we have developed methods of measurement and of statistical analysis which depend on the same assumptions. Our procedures are aimed at isolating variables from their functioning personal and social contexts. We try to find pure variables, measures of unitary dimensions that will be uncontaminated by other variables. The ideal measures are independent, free-standing, orthogonal -- that is, they are unrelated to measures of other variables. It is clear from the history of psychometrics that it is possible to do this, or at least to create the illusion that there are variables that are both independent and pure. It is much less evident that such variables have brought us any closer to the general laws they were intended to serve or to a deeper understanding of human action.

The rationale for all this work, much of it highly imaginative and sophisticated, is that we have been doing science. The rules of experimental design, the technical procedures of measurement and statistical analysis, are the time-tested methods of the biological and natural sciences. Their demonstrated success in those fields have justified their application to the study of persons, groups, social processes and social institutions.

The possibility that this specific formulation of the scientific method might be inappropriate to the study of psychological and social phenomena has not gone un-noticed. There is a long and a rich debate about this among philosophers of science, which I will not review here. My point of departure is the recognition that, despite the philosophical critique, the positivist model of science applied to human affairs remains triumphant. The research methods, which I've referred to collectively as context-stripping procedures, are taught to us in our graduate schools. We become properly certified as psychologists, sociologists or educational researchers when we can demonstrate our competent use of them in our dissertations. We build our reputations by publishing reports of studies based on these methods and/or by critical reviews of the research of others whose grasp of these methods seems less firm and competent than our own.

The ineffectiveness of the philosophical critique of positivist science reminds me of the fable of the condemned man which I first heard in graduate school. A judge sentenced the condemned man to be hung during the week following sentencing but added the proviso that he was not to know the morning on which he was to be hung. The condemned man immediately demanded his freedom. He pointed out, with impeccable logic, that if the last morning of the week arrived and he had not yet been hung, then he would know

it was to be that morning; hence, he could not be hung then. Extending his logic backwards through each preceding day of the week, he proved that since he would always know which morning it was to be, then the sentence could not be carried out. The judge listened politely. They took him out and hung him the next morning.

The instructor who told us this tale was making the point that the philosophical critique of positivist science had had as much effect on psychological and social research as the logic of the condemned man had on stopping the wheels of justice from turning. The moral we might draw is that what is logically impossible to do can still be done.

There are many reasons for the perseverance, or what sometimes seems like the perseveration, of the traditional model of science. I shall return to some of them in my concluding section. As a way of raising questions about the model, with particular reference to the problem of context, I'll begin by reviewing briefly a recent stream of criticism which is based more on empirical findings and less on logic and philosophy.

More precisely, this criticism comes from investigators who have used the prescribed methods and found them wanting. They have been disappointed by the results. In their well-known and influential monograph on quasi-experimental designs in educational research, Campbell and Stanley (1963), suggest that this disappointment is the result of overoptimism. Researchers and practitioners thought that progress would come quickly, as soon as the scientific method was applied. Campbell and Stanley note that unfortunately, "...the overoptimistic grounds upon which experimentation had been justified were undercut, and a disillusioned rejection or neglect took place... (when experiments) often proved to be tedious, equivocal, of undependable repli-

cability, and to confirm prescientific wisdom." (Op. Cit., p. 3) These authors remain faithful to the ideal value and ultimate justification of experimental research which they refer to as "...the only available route to cumulative progress," the "basic language of proof," and the "only decision court for disagreement between rival theories." (Op. Cit., p. 3)

Campbell and Stanley's continued faith and optimism comes with the recommendation that we adopt a "more mature focus". Namely, that we recognize that critical experiments are unlikely, that experiments are likely to produce mixed results rather than crucial tests of competing hypotheses, and that the balance of "truth" will vary from experiment to experiment. In their effort to shore up the courage and commitment of other investigators, they are restating an old argument. Ours is a young science, so this argument goes. It was a mistake, a sign of over-optimism, to think that knowledge and theory as systematic and precise as that in the biological and natural sciences would come easily and quickly. In this view, it is still too early to fault the method.

This argument has been countered recently by Sigmund Koch (1975). He begins by quoting John Stuart Mill, writing 135 years ago, in 1743: "The backward state of the (psychological) sciences can only be remedied by applying to them the methods of physical science, duly extended and generalized." (Op. Cit., p. 484) Koch then asserts, "The Millian hypothesis has been under test for nearly a hundred years. It has been tested in billions of man-hours of research and of ardent theoretical thinking, scholarship, writing, planning, and administration. It has been tested in hundreds of laboratories by many thousands of investigators. ...The test has not been a sleazy one. In my estimation, the Millian hypothesis

has been abundantly disconfirmed. I think it by this time utterly and finally clear that psychology cannot be a coherent science, or indeed a coherent field of scholarship, in any specifiable sense of coherence that can bear upon a field of inquiry." (Op. Cit., p. 490)

As many of you know, Koch is not speaking out of ignorance. He is not an outsider throwing barbs at an easy target. He was the senior editor of the massive undertaking by the psychology establishement in the 1950's to systematize existing knowledge and theory in the various domains and fields of psychology. The series of six volumes, published under the collective title, Psychology: A Study of a Science, have lacked their capstone. This was to have beer Koch's postscript volume, summarizing and synthesizing the present state of knowledge and theory. The brief quotation from him on the disconfirmation of the Millian hypothesis may suggest why this volume has never been published.

Other critics, also from within the field, have been more specific in pointing to the source of the problem and their comments will help bring us back again to issues of context. Gergen (1973), for example, has set off a still-continuing debate in social psychology by arguing that the laws found in both experimental and field studies are bound to their historical contexts and that the search within social psychology for trans-historical laws is illusory.(See Symposium, 1976) He asserts that "If we scan the most promising lines of research during the past decade, we soon realize that the observed regularities, and thus the major theoretical principles, are firmly wedded to historical circumstances." (Op. Cit., p. 315) In a later paper, as evidence for this statement, he cites the finding that prior to 1958 studies of selective learning and retention of

attitude-consistent information provided support for this hypothesis, but that since 1963 the original findings have not been replicated. There is a similar lack of reliability of findings over time in such areas as mass vs. distributed learning effects, aptitude-treatment interactions, and level of aspiration studies. (1976, p. 378) Gergen concludes, "... the continued attempt to build general laws of social behavior seems misdirected, and the associated belief that knowledge of social interaction can be accumulated in a manner similar to the natural sciences appears unjustified." (1973, p. 316)

Bronfenbrenner's critique (1977) of research in developmental psychology is similar in tone and emphasis. He demonstrates, through the comparison of different studies with each other, that reported and well-accepted findings on cognitive and social development are context-specific rather than having the generality claimed by investigators. For example, he reports that comparative studies of behavior in home and laboratory settings show that "...patterns of parent-child interaction in the laboratory are substantially and systematically different than those in the home. ...so far as young children are concerned, the results indicates that the strangeness of the laboratory situation tends to increase anxiety and other negative feeling states and to decrease manifestations of social competence." (Op. Cit., p. 516)

He proposes a framework of social contexts, from macro- to micro-, that is, from cultures and social institutions through communities and primary groups to laboratory settings, and urges the inclusion of multiple contexts in research for the proper evaluation of the validity and generalizability of indings. There is a further critical idea to be found in

Bronfenbrenner's analysis which is that the experimental laboratory is not the pure context-free setting assumed by the traditional model. That is, the experimental laboratory is a particular type of social setting or "ecological context" with its own specific context effects. For example, the laboratory is probably always seen by children as a strange environment, and this makes their behavior there an instance of behavior in "strange" situations. Thus, it cannot be assumed that findings from an experimental situation are any more context-free or universal than findings from other situations. (Martin Orne has coined the term "demand characteristics" to refer to these factors in experimental situations and has detailed a number of ways of detecting, specifying, and controlling them, 1969).

Abelson's comments (1977) on research into cognitive processes, attitude change, and decision-making also focus critically on the lack of attention to context in these areas. "Apart from criticism of specific results," he states "the classical experimental orientation can be questioned because hypothetical vignettes are so often presented without context. The consequent congitive processing is quite possibly very special, and not at all representative of naturalistic processing based on familiar scripts." (Op. Cit., pgs. 7-8) He notes the implicit biases of experimental work in cognition and decision-making, where, in order to avoid the contamination of real issues in subjects' lives, stimulus materials are used which consist of "...contextless sentential propositions expressing amiable foolishness. ...In standard experimental designs, we find counterbalanced foolishness where arbitrary subjects and predicates are factorially varied. Exclusive reliance on them (i.e., silly sentences) guarantees that no attention will be paid to pragmatics." (Op. Cit., pgs. 7-8)

The fact that we know very little about the psychology of natural decision-making comes, he suggests, from our "...reluctance to theorize about concrete rather than abstract decision-making processes." (Op. Cit., pgs. 11-12) And finally, he attributes the "mess" of current attitude theory, with its difficulty in accounting for situational variance and its failures to find attitude measures which correlate with overt behavior, to the assumption of experimentalists that there is a reality to the abstract predispositions they study, predispositions which they discover by having subjects respond to objects not encountered in ordinary experience.

In the face of such negative findings, what would keep experimenters at their appointed tasks? McQuire (1973), in a wide-ranging critique of traditional methods of research in social psychology suggests one possible answer. Laboratory experiments are not designed, he argues, to test carefully formulated theoretical hypotheses but to demonstrate the experimenter's ability as a stage manager. "...what the experiment tests is not whether the hypothesis is true but rather whether the experimenter is a sufficiently ingenious stage manager to produce in the laboratory conditions which demonstrate that an obviously true hypothesis is correct." (Op. Cit., p. 449) The sense of accomplishment generated by such experimental successes, and the professional rewards which may follow, are real enough. The experiment, conceived originally as a means to the end of scientific knowledge -- as the "only available route to cumulative progress" -- may come to be an end in itself. The lack of real cumulative knowledge becomes evident only through the reflective review of work on particular topics, such as those undertaken by the critical commentators to whom I have referred.

The positivist model of science remains dominant in our field, but the ranks of the critics appear to be growing. The few whom I've cited are not alone; hardly a month passes without another critical broadside focused on research in one or another discipline or sub-field. To deflect the anticipated charge that concern about such problems as context-effects is the province of arm-chair philosophers, I selected commentators who had strong and credible credentials as researchers. Unfortunately, such comments may discourage research by making it appear impossible, or at least extraordinarily difficult to do. Historical contexts change with remarkable rapidity and there is an inexhaustable multiplicity of social contexts. If scientific "laws" are context-dependent in the sense in which Gergen, Bronfenbrenner and others suggest then the traditonal goal of science may be unattainable. I think this is a valid implication if the problem is formulated as it has been traditionally, namely, as a search for laws which describe the relationship between two variables. Our theories and associated methods have all been directed to this end, namely, to the discovery of laws of the form $X = f(Y)$.

To take one example, because it so clearly articulates this approach, Campbell and Stanley (1963) examine in detail the sources of internal and external invalidity in all the standard experimental and quasi-experimental designs. The principle aim of their exercise is to specify "plausible" rival hypotheses, that is, factors other than the experimental variable "Y", and to indicate how different experimental designs control for these factors. Thus, they state, "...the list of sources of invalidity which experimental designs control can be seen as a list of frequently plausible

hypotheses which are rival to the hypothesis that the experimental variable has had an effect. Where an experimental design "controls" for one of these factors, it merely renders this rival hypothesis implausible ... Rival hypotheses are plausible insofar as we are willing to attribute to them the status of empirical laws. Where controls are lacking in a quasi-experiment, one must, in interpreting the results, consider in detail the likelihood of uncontrolled factors accounting for the results. The more implausible this becomes, the more "valid" the experiment." (Op. Cit., p. 36)

It is in the service of this task -- the elimination of plausible rival hypotheses to the preferred and explicit hypothesis in any particular study, that is, that the effect on a specific dependent variable "Y" is a function of a specific independent variable "X" -- that the experimental method is viewed as exemplary of scientific research and, in Campbell and Stanley's phrase, as "the only available route to cumulative progress." And, it is around this point that we may be able to clarify a critical issue which underlies the diverse and active critique of the traditional model of science, a critique I have illustrated by drawing upon comments of investigators in child development, cognitive psychology and attitude theory, and social psychology.

Put briefly, I would propose an alternative formulation of the task or goal of science in its search for general laws. The task is to specify the conditions under which a relationship holds of the form $X = f(Y)$. Traditonally, such laws always contained an implicit certeris paribus clause, that is, the assumption that the law held under the condition "all other things being equal." The task of experimental design is to control for, randomize, or in other ways reduce the effects of other

factors so that this assumption of "all other things being equal" could be accepted. If it is not acceptable, then neither the unequivocal interpretation of an experimental effect within a particular setting nor its generalization to other settings is possible.

But it is exactly this assumption that the critics have called into question. Other things are never "equal", however elegant the design. The experimental situation, even the aseptic laboratory, is not neutral but always reactive. And natural settings, however well they can be made to simulate quasi-experiments, are reactive in perhaps more dense and complex ways. Any regularity discovered through research which relates a patterned association between two variables is itself context-dependent.

It is to this problem that our efforts need to be directed. How can we formulate generalizations that make explicit the context-dependence of relationships? How can we develop methods - of observation, measurement, and analysis - that are appropriate to the study of such context-dependent "laws"?

Bronfenbrenner's recommendation that research be conducted in multiple contexts (1977) is generally useful advice, but is limited by its continued adherence to the traditional aim. That is, the reason given for studying multiple contexts is to test the range of situations in which a generalization holds; this is not the same as specifying the contextual conditions under which a relationship holds or does not hold. In the former approach, there is continued reliance on the assumption that laws of the form $X = f(Y)$ are possible and are the aim of research.

There is another important assumption in both the traditonal model and in recommendations like Bronfenbrenner's that studies be conducted in

a variety of contexts. This is that measurement of the variables "X" and "Y" be standard from situation to situation. If this were not so, then it would be difficult to determine whether a generalization applied across contexts. There are other approaches, as we shall see below, which involve a more radical departure from the traditonal model in which this assumption of the standard measurement of variables is also called into question.

In the search for more adequate models and methods of research, more adequate to the context-dependence of measurement and of relationships between variables, it is useful to turn to approaches where the study of situated meaning has been central. We will focus on three in order to illustrate how problems of theory and research may be reformulated for systematic study. These are: phenomenology, sociolinguistics and pragmatics, and ethnomethodology.

In a pair of related monographs, Carini (1975) and Patton (1975) have contrasted the positivist model of science with a research paradigm based on a phenomenological perspective. Their analyses are particularly useful for our purposes since they focus on problems of evaluation research in education. Carini provides a set of polarities which distinguish the two approaches. For example, in the positivist tradition the observer is outside of and independent of the observed phenomenon. The phenomenon itself is defined in terms of the observer's categories and its objective characteristics, namely, what is true about it, are to be discovered through application of traditonal methods of systematic inquiry. In the alternative phenomenological tradition, the perspective of the observer is intertwined with the phenomenon. It does not have objective characteristics independent of the observer's perspective and methods. Further, it contains multiple

truths, each of which will be revealed by a shift in perspective, method, or purpose. That is, since reality is knowable in an infinite number of ways there can be many equally valid descriptions. The choice among them depends on the purposes of the investigator and on the focus of the investigation.

Patton's analysis is complementary to Carini's. He provides a more specific set of contrasts such as the emphasis in the dominant scientific paradigm on quantitative methods, reliability of measures, the analysis of components or parts, and on outcomes. This is concrasted, respectively, to an emphasis on qualitative methods, validity, holistic analysis, and process in the alternative phenomenological approach.

Carini (1975) describes a method of observing, recording, describing and analyzing human phenomena, called documenting. (See also Engel, 1975) It involves a "...process of selecting and juxtaposing recorded observations and other records of phenomenal meaning in order to reveal reciprocities and therefore to approach the integrity of a phenomenon. (Op. Cit., p. 29) Since the task is not to "exhaust" the singular meaning of an event but to reveal the multiplicity of meanings and since it is the observer's encounter with the event through which these meanings emerge, no standard or general format for collecting observations would be appropriate for different settings and different purposes. Rather, a variety of procedures are used including graphs and charts of children's activities in various classroom spaces, records of interaciton patterns among children, teachers' notes, and children's products. The process of documenting may move through several levels, which are not so much steps to be taken one after the other

but rather a movement toward a fuller, more elaborate and complex understanding of the multiple meanings of the phenomenon, of its coherence, durability and integrity.

A principal aim of this process is the specification of the "enduring reciprocities" that constitute relevant meanings of the events. These permit the development of "matrices", or collections of significant dimensions. For example, in her illustration of dimensions of the reading process, Carini includes different modalities of expression, whether diffuse or articulate, and of the strength or weakness of visual and auditory organization. These matrices both guide and refine continued observation, but may also be used in a further stage of descriptive research. In the latter, the matrices may be subjected to verification by observations in settings which have been limited and specified by its dimensions. This would not appear to differ significantly from the more traditonal natural field experiment, but the emphasis is still placed on further documenting and on an exploration of the limits of the original matrices rather than on the definitive testing of hypotheses.

Carini's monograph, and other related reports produced by the North Dakota Study Group on Evaluation, is particularly useful since, in addition to outlining some of the critical philosophical and methodological differences between the positivist and phenomenological approaches, she also specified a set of methods for doing research within the latter framework. In discussing the general problem of context with other investigators, I have often found that the question of method is central. "Well, that's all very well," they might say, or "The argument makes sense," but

"How can you study that? What do you do next? How could I report what I've done?" The method of documentation is one specific answer to these questions.

The method of documentation is similar in several important respects to ethnography, an approach which has been suggested as particularly suitable for the study of teaching and learning in classroom settings. (See Magoon, 1977 and Wilson, 1977) The similarities include an emphasis on observations in natural settings, on what Geertz has called "thick descriptions" (Geertz, 1973), and on participants' perspectives or definitions of the situation. There is no special problem in seeing documentation as one type of ethnography, but at the same time it should be clear that the aim of documentation is not a full or comprehensive description but an analysis of the "reciprocities of meaning" that constitute the dialectical unity of the phenomenon of interest. That is, documentation is a method of analysis and not simply a list of procedures for recording field notes.

For students of language, particularly for sociolinguists, problems of context have come, in recent years, to be central and pervasive. This marks a change from the earlier research tradition that was dominated by a search for a universal grammar. In this tradition, languages were defined as systems of grammatical features and rules, and questions of meaning and of use, that is, of semantics and pragmatics, were either ignored or treated as of secondary interest. This period now seems to be ending.

The linguist's intuitive judgement of the grammaticality of sentences turned out, on closer inspection, to be a weak criterion. Linguists do

not always agree among themselves, and a number of studies have demonstrated the context-sensitivity of such judgements. (Labov, 1972) Bates (1976a) reviews efforts to incorporate contextual considerations into linguistic theory through such notions as performatives, pre-suppositions and conversational postulates. Not all developmental psycholinguistics are likely to agree with her assertion that "all of language is pragmatic to begin with" and that studies of the acquisition of pragmatics "indicate that semantics emerges,developmentally and logically, from pragmatics, in much the same way that syntax has been shown to emerge from semantic knowledge." (Op. Cit., p. 420) But the confidence with which Bates makes this statement clearly marks a significant change of direction in language research. (See also Bates, 1976b)

One issue of particular interest is how sociolinguists formulate context-dependent rules. A rule is one type of generalization or law, and we have noted that in the traditional model of science the essential criterion of a scientific law is that it be context-free. Labov and Fanshel's research (1977) on psychotherapy as discourse explicates one approach to this problem.

They state: "Eventually we must construct discourse rules that are quite general and stand free of context; but with our present state of knowledge, the only way we can be sure that these rules are developing in a useful direction is to show that they apply over and over again in many different contexts. In any particular context, we will not know if a particular rule applies unless our knowledge of contextual conditions is accurate." (Op. Cit., p. 73)

As a general aim, this does not seem too different from the more traditional approach to generalizations. Nonetheless, we find an important

difference when we examine Labov and Fanshel's particular rules of discourse. For example, their most general rule, a Rule of Requests, specifies the conditions under which a hearer will understand a speaker's utterance as a request for action on the hearer's part. They assert that like rules of syntax, such rules of discourse have an unconscious, invariant character and are compelling and obligatory. By obligatory, they mean "...that the speaker has no choice but to interpret a given action in the same way that all members of society would." (Op. Cit., p. 75)

Obligatoriness is a more stringent criterion than any of the usual criteria applied to generalizations or laws in the psychological and social sciences. Where then does context come in? Context comes in at the point where the conditions are specified under which a rule holds. For example, when we examine the explicit definition of the Rule of Requests, we find the necessary conditions all refer to situationally-grounded and situationally-understood beliefs of hearers with regard, on the one hand, to their own abilities and obligations to carry out the requested action and, on the other hand, to the speakers' needs, purposes, and rights in the situation. Thus, one condition for an utterance to be heard as a valid request for action is that the hearer believe that the speaker would not expect the hearer to undertake the action unless requested to do so. Obviously, this condition requires full understanding by the hearer of the context, including the temporal course of the interaction from its past into its expected future.

The important point is that this invarient, context-free rule is contextually-grounded. Labov and Fanshel are not asking how general this rule is; they assert that it is invariant and universal. They would not

study multiple contexts, as Bronfenbrenner has proposed, in order to determine its range of generalization. Rather, they would study other contexts to determine if the conditions were present under which the rule applies and, in order to do this, they would need "accurate" knowledge of the contextual conditions.

Similar considerations apply if we ask how the rule is to be tested. Such a test would not require a comparison of different settings, of the home vs. the classroom, or the laboratory vs. a natural situation. Rather, the test of the rule in any setting would involve a determination of whether an utterance was heard as a valid request in the absence of any of the proposed necessary conditions for an utterance to be heard in this way. Or, alternatively, whether an utterance was not heard as a valid request when all conditions were present. We would have to specify the types of evidence in the language or action of hearers which indicated that the utterance was or was not treated as a valid request. For example, we might look to the compliance of the hearer, or to his/her active noncompliance or refusal to comply.

On last point about these rules is that they have the form which I suggested earlier as an alternative to the traditonal two-variable law, $X = f(y)$. The Labov-Fanshel Rule of Requests does not state a relationship between some feature of a speaker's utterance and the hearer's understanding of it. Rather, it states that such a relationship will be present, namely, that an utterance will be heard as a valid request, under specified contextual conditions. This is a generalization or law whose context-dependence has been made explicit.

I've discussed several different ways in which investigators have been attempting to take into account the context-dependence of both the measure-

ment of variables and the lawful relationships between variables. Each represents a departure from the assumptions and prescriptions of the positivist model of science. I've noted some of their principal recommendations for research: the use of multiple contexts, the method of documentation, the specification of contextually-grounded rules of linguistic use. There are other approaches which depart even more significantly from the traditional model. These are represented most explicitly in two new developments, semiology and ethnomethodology.

In both semiology and ethnomethodology, the distinction between subject and context is dissolved. Meaning is always within context and contexts incorporate meaning. Both are produced by human actors through their actions. Semiologists speak of the"productivity of new meanings" in discourse and of the "chain of signification" in which meanings are located. (See Coward and Ellis, 1977) Ethnomethdologists focus on the "practices" by which social actors make their actions accountable and understandable, that is, meaningful in context. (Garfinkel, 1967)

In a seminal paper, Garfinkel and Sacks (1970) argue that the "earmark" of all practical sociological reasoning, whether done by professionals or by members of society in the course of their everyday lives, is the effort "...to remedy the indexical properties of practical discourse." (Op. Cit., p. 339) The problem of indexicality is the mirror-image of context-dependence. Among philosophers, an indexical expression has been defined as a word, sentence, or proposition whose meaning depends on the context in which it appears. These expressions have been a source of much difficulty, particularly for logicians and philosophers of language, since general theories about and procedures for determining the truth or falsity

of propositions cannot be applied to them. The pronoun "it", and any proposition containing this term, is often used as an examplar of an indexical since we cannot know its meaning without knowing the occasion and context of its use. Garfinkel and Sacks make the radical proposal that indexicality is an essential feature of all acts and utterances. It is for this reason that the "earmark" or practical sociological reasoning is the remedying of indexical properties. They argue that if all meaning is, in this sense, situationally-and contextually-embedded, then the principal topic of research should become the study of these "remedying" practices through which we make sense of each other in the ongoing course of our talk and action.

In all of the alternative approaches to the context problem which I reviewed earlier, there is an implicit assumption that the context is independent of the actor. This is most clear for the ecological psychologists and the recommendation that behavior be studied in multiple contexts reflects this assumption. Even for the sociolinguists, although the rules of use incorporate contextual conditions, the rules are defined as obligatory and compelling. The emphasis is on the side of the rules "causing" the action.

Ethnomethodology takes the apparent invariance of the rules, the features of obligatoriness and compellingness, as the topic of study. It is not that actors are governed by rules, but that they are users of rules. The invariance of a rule, or its applicability in different contexts, is not an explanation of speakers' behaviors but is, in Garfinkel's phrase, their practical accomplishment. That is, its invariance across contexts

is a product of members' work. Action and context are not independent of each other, but are reflexive, in that meaning is produced simultaneously with context. In similar vein, the semiologists refer to the "signifying chain" of discourse which is both the context within which components have meaning and whose meaning is itself given by the components.

This brings me to what may sound, at first, like a form of special pleading, a request that ethnomethodology by exempted from standard requirements of reporting and communicating. I shall try to indicate that what is involved is an even more stringent requirement than is usual, but that this poses serious practical difficulties.

Briefly, the problem is that ethnomethodological research can neither be done quickly nor can its results be reported adequately in the form of compact summaries. Reading an Abstract of an ethnomethodological study can be frustrating since it is likely to be very vague and/or highly abstract; trying to prepare one is even more frustrating. This problem is shared by the other approaches I've been describing of documentation and sociolinguistic research. The method of documentation clearly requires more time and attention on the part of an investigator than the availability of an adequate supply of machine-scorable rating forms. The Labov-Fanshel monograph to which I've referred may be somewhat extreme in these respects, but it may serve to suggest the nature of the problem. Their book is almost 400 pages in length. It reports analyses of one 15-minute stretch of talk in one therapy session; they note in passing that the research took eleven years to complete.

The world is not nearly as just as we would like it to be and hard work does not guarantee the scientific merit of a study. Nor, do I wish to imply that other investigators do not work equally hard. However, over eleven years, working with the more familiar quick-and-dirty in-and-out methods another investigator might have completed five or six reasonably complicated studies.

Since I would like to encourage you to think about the study of meaning in context through the use of one or another alternative approaches, it may be unwise to bring this problem to your attention. Nonetheless, it is necessary that you appreciate this problem since it helps to explain why the presentation of findings from ethnomethodological research is so difficult. If I report examples of findings to you, there is a strong sense in which it would be necessary for you to observe the phenomenon itself in order to assess the validity and meaningfulness of my report. Otherwise you would have to take my interpretation on faith. This may lead, and often does, to a misunderstanding of the grounds of inference, of the basis of an interpretation. It may appear that interpretations are vague intuitions, subjective and speculative. Nothing could be further from the mark. The findings and the interpretations reflect detailed analyses, as I've tried to suggest by the Labov-Fanshel example. But these findings are not represented in correlation matrices or significance tests. Even the Labov-Fanshel Rule of Requests, which is stated in an abstract and formal way, requires a return to the audio-tape and the transcript in order to properly assess its validity and applicability.

As another example, I have been studying how first-grade children do trading. (Mishler, 1978) I have claimed that a rule or strategy of

bargaining which they use is to avoid making the first offer to trade. And further, that this strategy is part of a bargaining process in which those who make first offers to trade lose bargaining power. If you are skeptical of this finding and ask for documentation or "proof", I would have to take you through those stretches of discourse that represent bargaining in order to show you how this rule is realized in the sequence of exchanges. I would have to show you how the interpretation was grounded in specific features of their language, including intonations, hesitations, recyclings of offers, and ways of terminating transactions. Clearly, this would require a different context than the present one.

Although I would prefer to leave you with a solution rather than with a problem, I hope I've been able to indicate why the preferred alternative is not practical in this setting. I'd like, in conclusion, to bring together the strands of the argument and, at the same time, untie some threads and leave them loose and hanging.

Concluding Remarks

Opening up the problem of context is like opening up Pandora's Box. Questions that had been "settled" once and for all by the scientific revolution reappear to haunt and bedevil us. The scientific method was to be our guarantor of objectivity and neutrality and diligent application of its rigorous procedures was, in turn, to produce a body of cumulative knowledge that was systematic and general. It has not worked out this way. At least, that is a central argument in this essay.

There is a crisis of confidence among investigators in the social and psychological sciences which I have illustrated by reference to several critics of research in such fields as child development and social psychology. A prominent theme in these recent critiques is the discovery that research findings appear to be context-dependent. For this reason, the search for lawful generalizations that would be cross-contextually valid has proved to be more difficult than expected and, some of the critics argue, may be a misdirected search. I have argued further that the context-stripping methods of our traditional model of science may be inappropriate to the study of context-dependent phenomena. I have also briefly described a number of alternative approaches and methods that may be more appropriate. These include methods drawn from ecological psychology, phenomenological research, sociolinguistics and ethnomethodology. These vary among themselves, and each has its own problems, but they provide a range of possibilities for us to rethink our ways of doing scientific research. I must also make clear that these criticisms and recommendations apply to my own work as well as that of other investigators. My examination of these issues reflects an effort to rethink the approach and direction of my own research

which has been no less committed to the traditional model of science than that of other researchers.

Scientific research is itself a social enterprise and subject to a variety of social forces besides its own declared imperative, which is a search for valid empirical knowledge and general understanding. The traditional model of science and its associated context-stripping methods has been useful for certain social purposes. Standard scales of measurement and statistical methods for the analysis of group differences have provided information that has been useful to administrators, managers, and policy-makers. The IQ test and all its sub-variants provide distributions of scores that allow school officials to sort children into different curricula. Tests of significance produced through the analysis of variance inform us that, on average, one method of instruction may increase reading scores more than another; therefore, justification is provided for introducing the more effective method into all classrooms. These are all useful social and practical purposes, particularly if you are an administrator, but they should not be confused with the aims of science.

Administrators would find the method of documentation less useful, but this does not mean that it is less scientific than a behavior rating scale. It is well-known that the subjects of our research usually cannot recognize themselves in our research reports. There are also well-known ways in which we justify this discrepancy between our understanding and theirs. It might, on the other hand, be considered a process of mystification through which scientists hide the meaning of their work through their control over esoteric methods and concepts. The methods are also mystifying to young investigators and often alienate them from their own work. I am sure that you have all had the experience of having students

and younger colleagues complain about how they can't find methods to study the problems in which they are interested. All too often we help them reshape their problems so that available traditional methods can be used. In the process, the original problem is often lost.

Science is not a cure, or even a palliative, for alienation. Nonetheless, it need not add to other alienating forces in the society. A better fit between our research methods and our phenomena of interest, namely, the context-dependence of human meaning and action, might be a first step toward a non-alienating science.

REFERENCES

Abelson, Robert P. "Script Processing in Attitude Formation and Decision-Making," In J.S. Carroll and J.W. Payne (Eds.) Cognition and Social Behavior. New York: Lawrence Erlbaum, 1976.

Bates, Elizabeth, "Pragmatics and Sociolinguistics in Child Language," In Donald M. Morehead and Ann E. Morehead (Eds.) Normal and Deficient Child Language. Baltimore, MD.: University Park Press, 1976a.

Bates, Elizabeth, Language and Context. New York: Academic Press, 1976b.

Bronfenbrenner, Urie. "Toward an Experimental Ecology of Human Development," American Psychologist, 1977, 32(7): 513-531.

Campbell, Donald T. and Stanley, Julian C. "Experimental and Quasi-Experimental Designs in Educational Research," In Nathan L. Gage (Ed.) Handbook of Research on Teaching. New York: Rand McNally, 1963.

Carini, Patricia F. Observation and Description: An Alternative Methodology for the Investigation of Human Phenomena. North Dakota Study Group on Evaluation Monograph, Grand Fords, N. Dakota: U. North Dakota Press, 1975.

Coward, Rosalind and Ellis, John, Language and Materialism. London: Routledge & Kegan Paul, 1977.

Engel, Brenda S. A Handbook on Documentation. North Dakota Study Group on Evaluation Monograph. Grand Forks, N. Dakota: U. North Dakota Press, 1975.

Garfinkel, Harold, Studies in Ethnomethodology Englewood CLiffs, N.J.: Prentice-Hall, 1967.

Garfinkel, Harold and Sacks, Harvey, "On Formal Structures of Practical Actions," In J.C. McKinney and E.A. Tiryakian (Eds.) Theoretical Sociology: Perspectives and Development. New York: Appleton-Century-Crofts, 1970.

Geertz, Clifford, The Interpretation of Culture. New York: Basic Books, 1973.

Gergen, Kenneth J. "Social psychology as history," Journal of Personality and Social Psychology, 1973, 26(2): 309-320.

Gergen, Kenneth J. "Social psychology, science, and history," Personality and Social Psychology Bulletin, 1976, 2(4): 373-383.

Koch, Sigmund, "Language Communities, Search Cells, and the Psychological Studies," In William J. Arnold (Ed.) Nebraska Symposium on Motivation: 1975. Lincoln, Nebraska Press, 1976

Labov, William, "The Study of Language in its Social Context," in William Labov, Sociolinguistic Patterns. Philadelphia, PA.: U. Pennsylvania Press, 1972.

Labov, William and Fanshel, David, Therapeutic Discourse. New York: Academic Press, 1977.

REFERENCES

Magoon, A. Jon, "Constructivist approaches to educational research," Review of Educational Research, Fall 1977, 47 (4): 651-693.

McQuire, William J. "The Yin and Yang of progress in social psychology, Journal of Personality and Social Psychology, 1973, 26(3): 446-456.

Mishler, Elliot G. "'Wou' you trade cookies with the popcorn?' The talk of trades among six year olds," in O.K. Garnica and M.L. King (Eds.) Language, Children and Society. New York: Pergamon Press, 1978 (in press)

Orne, Martin T. "Demand Characteristics and the Concept of Quasi-Controls," In Robert Rosenthal and Ralph L. Rosnow (Eds.) Artifact in Behavioral Research. New York: Academic Press, 1969.

Patton, Michael Quinn, Alternative Evaluation Research Paradigm. North Dakota Study Group on Evaluation Monograph. Grand Forks, N. Dakota: U. North Dakota Press, 1975.

Symposium: Social Psychology and History, Personality and Social Psychology Bulletin, 1976, 2(4): 371-410.

Wilson, Stephen, "The use of ethnographic techniques in educational researcch," Review of Educational Research, WInger 1977, 47(1): 245-265.